A Comment on "Legalized Sunday Packaged Alcohol Sales and Alcohol-Related Traffic Crashes and Crash Fatalities in New Mexico" by McMillan and Lapham

by

M.T. Maloney & Jason Rudbeck
John E. Walker Department of Economics
Clemson University[1]
December 31, 2006

Analysis of automobile accidents and fatalities is an important policy issue and one that should not be taken lightly. The recent paper by McMillan and Lapham in the *American Journal of Public Health* argues that package alcohol sales on Sunday in New Mexico, initiated in 1995, increased alcohol related crashes and fatalities.[2] The purpose of this note is to explore the possibility that their results are merely a spurious result of their modeling technique.

The authors look at the number of alcohol related accidents and fatalities before and after the law allowing package liquor sales became effective in July 1995, and based on an estimated increase in the number of incidents in the post period, they conclude that this increase was a consequence of the law. In the statistical literature, this is called a difference estimator. It is a restrictive model because it lacks controls for other things that might be going on.

In this paper we control for those other things by using a difference-in-difference estimator. We look at the change in the number of alcohol related incidents pre- and post-law-change compared to the change in the number of non-alcohol related incidents. The non-alcohol related incidents are the control for unobservable factors causing all accidents—alcohol and non-alcohol—to increase or decrease. Based on our difference-in-difference estimation, we find no effect of Sunday sales of package liquor on the number of fatal crashes and the effect on the total number of crashes is statistically weak in the period from 1995 to 2000. This weak effect becomes statistically insignificant when the estimation period is extended through 2005.

A Simple Look at the Data

As a first blush look at this issue, we compare the simple sample counts of alcohol-related fatal crashes and non-alcohol-related fatal crashes on Sunday before and after the law was changed. Table 1 shows these numbers. There are two salient points. First, the number of Sunday non-alcohol fatal crashes increased more than the number of alcohol fatalities. There appears to be something about Sunday in the post Sunday-sales period that increased fatalities that is not related to alcohol. Whatever this is, it can reasonably be expected to affect alcohol fatalities as well.

---

[1] Maloney is Professor of Economics and can be reached at 864-656-3430 or maloney@clemson.edu. Rudbeck is a recent graduate of the PhD program. He can be reached at 864-656-3481 or jcrudbe@clemson.edu.

[2] G.P. McMillan and S. Lapham, "Effectiveness of Bans and Laws in Reducing Traffic Deaths: Legalized Sunday Package Alcohol Sales and Alcohol-Related Traffic Crashes and Crash Fatalities in New Mexico," *Government, Politics, and Law, American Journal of Public Health,* November 2006, 96(11) 1944-1948.

The second striking thing is that the increase in the number of Sunday alcohol-related accidents is so small. There were only four more fatal accidents in the post law-change period than the pre. It is very difficult to believe that these four accidents can be attributed with statistical confidence to a change in the law concerning the sale of package liquor. Whatever statistical analysis is done should be carried out with the full recognition that there is very little variation in the data. Standard practice in such cases is to fully explore the effects of specification sensitivity.

Table 1. Fatal Crashes on Sunday

	Alcohol	Non-Alcohol
Pre-Sunday-Sales	119	130
Post Sunday-Sales	123	146
% Change Pre-Post	3%	12%

Notes: Sunday defined as noon Sunday to noon Monday. Pre-Sunday sales is July 1990 through June 1995; post Sunday sales is July 1995 through June 2000.

Total crashes show a similar picture. Table 2 gives the average number of crashes on Sunday for all levels of severity. As the data show, the average number of Sunday crashes that did not involve alcohol went up in the post-period while the average number of alcohol related crashes went down. These are the raw data and need to be examined with controls, but they do give us a sense of the magnitudes of the effects that we are examining. There are not that many alcohol related crashes on Sunday, and the change before and after the law allowing Sunday sales of package alcohol is quite small.

Table 2. Average Number of Crashes on Sunday—All Types

	Alcohol	Non-Alcohol
pre-Sunday-Sales	9.8	95.0
post Sunday-Sales	9.2	98.0
% Chng Pre-Post	–6.0%	3.1%

Notes: See notes to Table 1. Crashes include fatal, personal injury, and property only.

An Alternative Approach

The data are crash incidents in the State of New Mexico taken from a database maintained by the Division of Government Research at the University of New Mexico. We have data from 1990 through 2005. First we will limit the sample to the same time period as analyzed by McMillan and Lapham, and then we will extend the analysis through 2005. Also, we have data on several other crash conditions. Importantly, we find that the type of roadway on which the crash occurred has a substantial impact on frequency.

The data that we use report 492,301 total accidents in the period July 1990 through June 2000. This compares to 492,396 reported by McMillan and Lapham. Also, our data show that 45,592 of these were alcohol related compared to 45,596 reported by McMillan and Lapham. The cause of these discrepancies is not known. The data are revised over time to increase reporting accuracy. At all events the discrepancies are minor and are unlikely to affect the results.

We look at accident counts by day for both fatal and non-fatal crashes, with and without alcohol involvement. Similar to McMillan and Lapham, we use a negative binomial estimation model.[3] We include a linear trend both for accidents overall and for alcohol-related accidents. In addition, we include year dummy variables for total and alcohol-related accidents to allow idiosyncratic variation around the trend line.[4] We include month dummy variables to allow for seasonal variation, dummy variables for the type of road (rural non-interstate, urban including interstate, and rural interstate), and dummies for holidays, day of the week, and the post law change period. All of these effects are interacted with alcohol-involvement and fatal crash where they are significant.

We estimate the model for all crashes, both alcohol and non-alcohol, while allowing for differences in the estimated coefficients. This is the essence of the difference-in-difference estimator. We want to investigate how different are alcohol related crashes compared to non-alcohol related crashes, and then see if this changes with the change in the law. Again, our concern is in part prompted by the facts that we have as shown in Table 1. Given the small sample sizes in the classification of primary interest (Sunday alcohol-related fatal crashes), we need to use as much data as possible to uncover the effect.

The estimated coefficients on the Sunday accident count with alcohol involvement in the post Sunday-sales period are shown in Table 3. Also shown is the standard error of the estimate, the probability that the coefficient is actually zero, and the marginal effect. The marginal effect is the estimated increase in the Sunday accident count with alcohol involvement in the post Sunday-sales period.

Table 3. Estimated Sunday Alcohol Involved Accidents in Post Sunday-Sales Period

	Coefficient	*Std Error*	*Prob(coef)=0*	*Marginal Effect*
All Accidents	0.083	0.052	0.113	1.086
Fatal Accidents	0.136	0.262	0.603	1.146

Notes: See notes Table 1. Estimates from negative binomial regression of accident counts of fatal and non-fatal accidents over the period July 1990 through June 2000. Regression includes controls for linear trend, year interacted with alcohol involvement, fatal crash, and alcohol and fatal together, month and month interacted with fatal crash, holidays and holidays interacted with alcohol, road type interacted with alcohol involvement, fatal crash, and the two combined, and interactions of weekdays, alcohol involvement, fatal crash, and pre/post the Sunday-sales law change. Observations: 43,836.

The first thing that we see in Table 3 is that neither coefficient on the post-law-change, Sunday effect is statistically significant at conventional levels. The coefficient on all accidents is close. The coefficient on fatal accidents is not. There is a 60 percent chance that the coefficient on fatal crashes is statistically zero, which confirms our suspicions from the casual look at Table 1. There

[3] The negative binomial which is a more general form of the poison used by McMillan and Lapham.
[4] Our approach is a more general version of the spline estimation used by McMillan and Lapham. However, our approach is less restrictive because it allows us to estimate separate year effectss for alcohol-related accidents, fatal accidents, and alcohol and fatal accidents. See the full model in the appendix.

are so few fatal, alcohol-involved, Sunday accidents that it is highly unlikely that an increase in the post law-change period is statistically present, and this is what the estimates indicate.

On the other hand, we do find a nearly statistically significant increase in the overall alcohol-related accident count on Sundays in the post-law-change period even though the raw count declined. This result is attributable to the trend effects which we will discuss in a moment. First, we need to examine the magnitude of the estimated effect. The marginal effect shown in the last column of Table 3 is the increase in the accident count—it is 1.086. McMillan and Lapham found an effect of 1.29. Our estimate is less than a third of their effect.

If we compare the estimated marginal effect of 1.086 to the average accident count shown in Table 2 it says that of the average 9.2 alcohol-related accidents on Sundays in the post-law-change period, only three-quarters of one accident can be attributed to any regime change that might have occurred as a consequence of the law change and then only with modest statistical confidence. Put differently, there were possibly three more alcohol-related accidents per month in the post-law-change period than can be explained by trend, seasonality, road type, and holidays.

Estimates from 1990 through 2005

Table 4 is the same as Table 3 except that we now include data from the beginning of 1990 through 2005. As shown there, both estimated coefficients measuring the frequency of Sunday alcohol-involved accidents, both fatal and non-fatal, in the post-law-change period are insignificant. The marginal statistical significance for the coefficient on total accidents that we found over the shorter estimation period goes away when we look at the longer period.

Table 4. Estimated Sunday Alcohol Involved Accidents in Post Sunday-Sales Period — 1990 through 2005

	Coefficient	*Std Error*	*Prob(coef)=0*	*Marginal Effect*
All Accidents	0.060	0.049	0.218	1.062
Fatal Accidents	0.095	0.244	0.700	1.100

Notes: See notes Tables 1 and 3. Estimates from negative binomial regression of accident counts of fatal and non-fatal accidents over the period January 1990 through December 2005. Pseudo R-squared: 0.95. Observations: 70,128.

All of the estimated coefficients from this regression are shown in the appendix. The pseudo *R*-squared of the regression, which we calculate by a simple OLS regression of accident counts on predicted counts, is 0.95.

What Makes Our Estimates Different from McMillan and Lapham?

Our estimation procedure differs from that of McMillan and Lapham on several margins, but the fundamental difference is the simultaneous estimation of alcohol and non-alcohol related crashes. Again, our estimates are difference-in-difference estimates where we are looking at the "treatment" effect of post-law-change, alcohol-related crashes relative to post-law-change, non-alcohol related crashes. This allows unobservable factors that might affect crashes in the post-law-change period to be included in the regression via the proxy of non-alcohol-related crashes. For instance, one of these unobservable changes is the increase in the speed limit which occurred in 1996.

Other differences in our estimation are the treatment of the time trend and the inclusion of road type. Road type has a significant estimated influence on accident counts. For instance, 3 times as many accidents occur on rural non-interstate and 18 times as many in urban areas than on rural interstates. Alcohol involvement is twice as likely on rural non-interstate as rural interstate and given alcohol involvement, fatal crashes are twice as likely on rural non-interstate and three times as likely on urban roads than on rural interstates. While these effects are large and demand inclusion in the regression, omitting these effects does not change the conclusions shown in Table 3.

The other way that our estimates differ from McMillan and Lapham is in the treatment of the time trend and seasonal effects. We use a linear time trend and then yearly fixed effects interacted with alcohol involvement and fatal crash. We also use month fixed effects and interact these with fatal crash. Various combinations of interactions between trend, year and month fixed effects and the alcohol and fatal crash indicators were tried. We retained the interaction effects that were statistically significant. These included year fixed effects interacted with both alcohol, fatal crash, and the two combined, and month fixed effects interacted with fatal crash.

We believe that our treatment of the time trend and seasonal effects is superior to that of McMillan and Lapham because it is unrestricted. Our use of fixed effects for year and month allows the data to speak directly without arbitrary specifications that are ad hoc. Moreover, our use of interactions between these fixed effects and the alcohol involvement and fatal crash indicators allows for the trend and seasonal effects to vary between the types of accidents while at the same time maintaining our difference-in-difference estimation procedure. Nonetheless, when we estimate the models using splines we get results nearly identical to those shown in Tables 3 and 4.

Conclusions

We do not claim that our estimates trump those of McMillan and Lapham, but we do think that their conclusions should be weighed against our results, which using nearly identical data show no evidence of a Sunday, fatal-crash effect following the legalization of alcohol sales on this day of the week. We do find a statistically weak effect for total crashes that is less than one-third the magnitude that they find, and this effect goes away when the estimation period is extended through 2005.

The fundamental difference in our approach compared to theirs is that we use a difference-in-difference estimator. We look at the effect of the law-change for alcohol related crashes controlling for the effect of the law change on non-alcohol-related crashes. This is fundamental as an estimating methodology. There are unobservable factors causing all crashes to vary through time. For instance, New Mexico changed its speed limits in 1996, an event almost coincident with the change in the law allowing sales of package alcohol on Sunday. The difference-in-difference estimator accounts for these unobservables; the simple difference estimator used by McMillan and Lapham does not.

We also use more information about the crash, in particular, the type of road where the crash occurred. Moreover, we take a more general approach to de-trending and de-seasonalizing the data. Both of these points are relative merits of our methodology and are generally the most flexible approach to implementing the difference-in-difference estimation.

At bottom, we are most impressed by Table 1 above. The fact is that fatal crashes on Sunday involving alcohol are such a low frequency event that much care must be taken in attempting to uncover a regime change associated with the law allowing Sunday sales of alcohol. We think that we have taken that care and find no evidence of an increase in alcohol-related fatal crashes on Sundays in the post-law-change period and little evidence of any change in alcohol-related automobile accidents.

Acknowledgement

The Distilled Spirits Council of the United States supported this research by a grant to the Clemson University Foundation for general graduate student funding.

Appendix

The following table shows the full set of estimated coefficients from the negative binomial regression from which the coefficients of primary interest are shown in Table 4.

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	*Std. Error*
Intercept	4.983	0.952
Trend	-0.858	0.249
Year Effects		
1990	-4.642	1.366
1991	-4.382	1.275
1992	-4.024	1.184
1993	-3.757	1.093
1994	-3.404	1.002
1995	-3.045	0.911
1996	-2.723	0.819
1997	-2.363	0.728
1998	-2.168	0.637
1999	-2.013	0.547
2000	-1.532	0.455
2001	-1.175	0.364
2002	-0.893	0.274
2003	-0.602	0.183
2004	-0.218	0.092
2005	0.000	0.000
Alcohol & Year Interacted		
1990	-1.810	0.033

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	Std. Error
1991	-1.751	0.033
1992	-1.721	0.033
1993	-1.668	0.033
1994	-1.744	0.033
1995	-1.768	0.039
1996	-1.857	0.052
1997	-2.043	0.052
1998	-1.920	0.052
1999	-1.989	0.053
2000	-2.041	0.053
2001	-2.067	0.052
2002	-2.040	0.052
2003	-2.033	0.053
2004	-2.169	0.053
2005	-2.323	0.054
Fatal Crash & Year Interacted		
1990	-3.650	0.120
1991	-3.552	0.119
1992	-3.719	0.122
1993	-3.665	0.122
1994	-3.585	0.119
1995	-3.489	0.140
1996	-3.597	0.184
1997	-3.585	0.183
1998	-3.524	0.184
1999	-3.352	0.184
2000	-3.484	0.183
2001	-3.441	0.182
2002	-3.589	0.184
2003	-3.647	0.186
2004	-3.362	0.181
2005	-3.302	0.181
Alcohol, Fatal Crash, & Year Interacted		
1990	1.713	0.149
1991	1.506	0.149
1992	1.582	0.151
1993	1.459	0.152
1994	1.326	0.151
1995	1.221	0.185
1996	1.409	0.250
1997	1.415	0.251
1998	1.271	0.252
1999	1.371	0.252
2000	1.354	0.252

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	*Std. Error*
2001	1.353	0.251
2002	1.609	0.251
2003	1.606	0.253
2004	1.331	0.250
2005	1.476	0.251
Month Effect relative to December		
January	-0.424	0.084
February	-0.429	0.076
March	-0.392	0.069
April	-0.375	0.062
May	-0.291	0.054
June	-0.270	0.047
July	-0.251	0.039
August	-0.197	0.032
September	-0.164	0.025
October	-0.110	0.018
November	-0.115	0.013
Fatal Crash & Month Interacted		
January	-0.110	0.070
February	0.057	0.069
March	0.061	0.067
April	0.169	0.066
May	0.150	0.065
June	0.282	0.063
July	0.268	0.064
August	0.286	0.062
September	0.218	0.064
October	0.217	0.063
November	0.078	0.066
December		
Holidays		
Super Bowl	0.012	0.051
St. Patrick's Day	-0.028	0.048
Cinco de Mayo	-0.022	0.047
Memorial Day	-0.146	0.050
4th of July	-0.078	0.049
Eve	0.012	0.047
Labor Day	-0.087	0.049
Halloween	0.031	0.046
Thanksgiving	-0.381	0.052
Eve	0.014	0.047
Christmas	-0.226	0.051
Christmas Eve	-0.177	0.048
New Year's Eve	-0.107	0.048

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	*Std. Error*
Alcohol & Holidays Interacted		
Super Bowl	0.246	0.103
St. Patrick's Day	0.248	0.095
Cinco de Mayo	0.243	0.093
Memorial Day	0.347	0.104
4th of July	0.575	0.088
Eve	0.384	0.088
Labor Day	0.333	0.101
Halloween	0.420	0.086
Thanksgiving	0.398	0.103
Eve	0.527	0.090
Christmas	-0.546	0.123
Christmas Eve	0.355	0.092
New Year's Eve	0.873	0.080
Fatal Crash & Holidays Interacted		
Super Bowl	-0.447	0.342
St. Patrick's Day	-0.412	0.311
Cinco de Mayo	-0.400	0.287
Memorial Day	0.410	0.233
4th of July	-0.078	0.221
Eve	0.231	0.193
Labor Day	0.386	0.219
Halloween	0.042	0.208
Thanksgiving	0.407	0.244
Eve	0.263	0.224
Christmas	0.341	0.269
Christmas Eve	-0.269	0.287
New Year's Eve	0.209	0.194
Road Type relative to Rural Interstate		
Rural Non-Interstate	1.221	0.008
Urban	2.888	0.007
Alcohol & Road Type Interacted		
Rural Non-Interstate	0.673	0.023
Urban	-0.103	0.022
Fatal Crash & Road Type Interacted		
Rural Non-Interstate	-0.932	0.043
Urban	-2.928	0.046
Alcohol, Fatal Crash, & Road Type Interacted		
Rural Non-Interstate	0.584	0.075
Urban	1.165	0.079
Day of Week Effects		
Day of Week relative to Saturday		
Sunday	-0.128	0.016
Monday	0.039	0.016

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	*Std. Error*
Tuesday	0.047	0.016
Wednesday	0.065	0.016
Thursday	0.095	0.016
Friday	0.244	0.016
Day of Week Post-Law-Change		
Sunday	0.044	0.024
Monday	0.047	0.024
Tuesday	0.047	0.024
Wednesday	0.025	0.023
Thursday	0.021	0.023
Friday	-0.034	0.023
Saturday	-0.054	0.024
Alcohol & Day of Week Interacted		
Sunday	-0.722	0.032
Monday	-0.878	0.032
Tuesday	-0.894	0.032
Wednesday	-0.748	0.031
Thursday	-0.710	0.030
Friday	-0.211	0.028
Alcohol & Day of Week Post-Law-Change		
Sunday	0.060	0.049
Monday	-0.132	0.049
Tuesday	-0.039	0.049
Wednesday	-0.156	0.048
Thursday	-0.101	0.048
Friday	-0.140	0.045
Saturday	-0.077	0.046
Fatal Crashes		
Day of Week relative to Saturday		
Sunday	0.092	0.119
Monday	-0.208	0.122
Tuesday	-0.345	0.127
Wednesday	-0.048	0.117
Thursday	-0.182	0.120
Friday	-0.176	0.116
Day of Week Post-Law-Change		
Sunday	-0.006	0.168
Monday	0.047	0.171
Tuesday	0.183	0.175
Wednesday	-0.181	0.168
Thursday	0.044	0.169
Friday	0.158	0.164
Saturday	0.105	0.167
Alcohol & Day of Week Interacted		

Table A1. Binomial Regression Estimates — All Coefficients

	Coefficient	*Std. Error*
Sunday	-0.049	0.161
Monday	0.210	0.163
Tuesday	0.172	0.172
Wednesday	-0.173	0.161
Thursday	0.063	0.159
Friday	0.104	0.144
Alcohol & Day of Week Post-Law-Change		
Sunday	0.095	0.244
Monday	-0.173	0.250
Tuesday	-0.015	0.255
Wednesday	0.331	0.248
Thursday	0.092	0.244
Friday	-0.114	0.230
Saturday	0.043	0.231
Dispersion	0.055	0.001
Pseudo R-Squared	0.950	